Suppose, now, that he is drifting towards B with a velocity equal to that of the wind, and travelling at right angles to AB with such a velocity that he can move along horizontally without falling towards the earth. Suddenly a gust overtakes him from the direction of A. He at once turns towards it, and his velocity relative to it is sufficient to raise him in the air. It tends to carry him more rapidly towards B; and when his velocity relative to it has sunk to the same value as before, and he again travels horizontally, he turns again at right angles to the line AB, but in the opposite direction to that which he had before. Presently the force of the gust diminishes, and the wind seems to blow towards him from the direction A. He accordingly turns towards it again, rising from the ground till his velocity relative to the air has assumed its former value, and he moves horizontally, turning again at right angles to the line AB, and the cycle is completed. He thus moves along in the direction AB with a mean velocity equal to that of the wind, rising when moving parallel to it, and moving horizontally, or perhaps slowly falling, if the gusts do not come with sufficient frequency, when moving at right angles to it.

In the case of all soaring birds, the spread tail, being an inclined curved surface, presents a large area to the wind. As it is situated at a considerable distance from the bird's centre of gravity, it must convert him into a sort of floating weather-cock, the wings serving as dampers to restrain him from turning too quickly. It therefore appears, if soaring really does depend on the interaction of varying wind-currents, as if the changes of direction involved must be almost automatic, and not a thing which the bird is required to learn; although he may doubtless learn to take advantage of favoring currents by giving proper inclinations to his wings and tail.

If the question be raised as to the sufficiency of the varying intensity of the wind-currents to maintain the bird's initial velocity against the resistance of the air, we must reply that it is a matter which can only be determined conclusively by experiment. Certain it is, however, that in windy weather the wind does come in gusts. If in the course of his circles the bird happens to be travelling at right angles to the wind, when the gust strikes him he will surely be turned round, almost in spite of himself, so as to face the gust. If the bird does face the gust, it will certainly raise him to a higher level.

If this explanation proves to be the true one, the reason why small birds cannot soar is probably, that, in those of them that have suitably shaped wings and bodies, their surfaces are so large in proportion to their weights that they rapidly assume the velocity of the surrounding air. In order that they might soar to advantage, the gusts should come more frequently, and be of shorter duration, than we actually find to occur in nature.

WM. H. PICKERING.

Harvard Observatory, Cambridge, Mass., March 21.

Definition of Manual Training.

I HAVE just seen in your pages (*Science*, xiii. p. 9) the excellent definition of "manual training," given by the New Jersey Council of Education. But the name is already too familiar in various vaguer uses, and especially for training to fit for manual labor: hence there would be great advantage if a fresh name were applied. Would not "manu-mental training" do admirably? It expresses the precise idea in such a way that a mistake as to its meaning is impossible.

J. E. CLARK.

Bootham, York, Eng., March 15.

Curves of Literary Style.

AFTER reading the communication on "Curves of Literary Style," in the last number of *Science*, I counted the words in 300 sentences towards the last of Carlyle's "French Revolution," and found the curve, when plotted, to agree very closely with your correspondent's as published, though there were several longer sentences interspersed, showing that the passages examined were from a different part of the work. This was very satisfactory; but the same method of examination, applied to the first 300 sentences of Carlyle's "Sartor Resartus," gives a very different result, the curve corresponding pretty closely with that given for Johnson's "Rambler." This goes to show, if it does not prove, that for detective purposes the method is valueless. All compound words and phrases connected by hyphens were counted as single words only. The 300 sentences filled 30 out of 200 pages of the edition used.

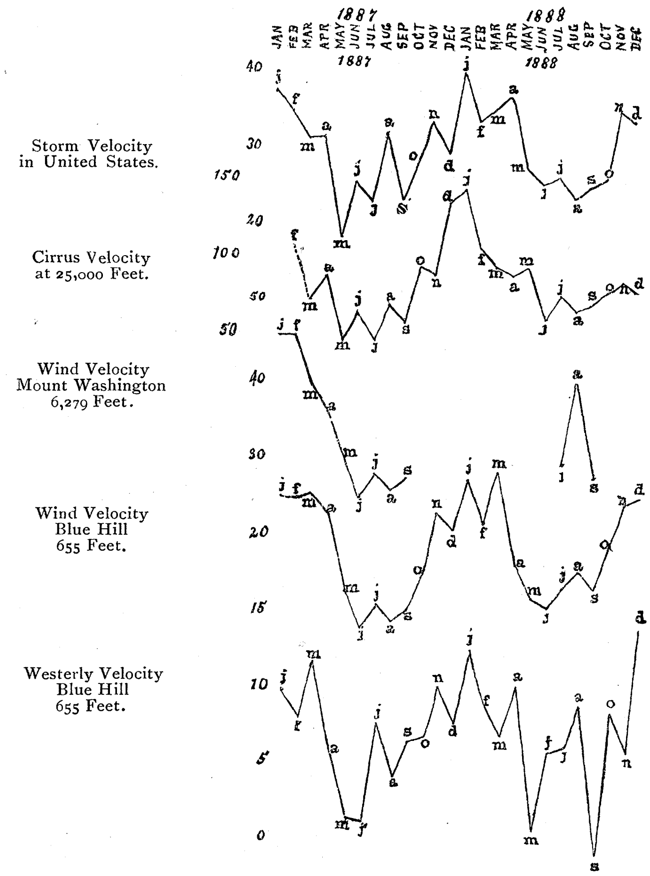
H. A. PARKER.

Cambridge, Mass., March 25.

The Velocity of Storms as related to the Velocity of the General Atmospheric Movements.

IT has for a long time been maintained by some meteorologists that the chief cause of the progressive movement of storms is that these atmospheric disturbances are carried along by the general movements of the atmosphere, as eddies on the surface of a river are borne along by the current in which they exist. The German meteorologists Van Bebber and Köppen have especially insisted on these views, maintaining that the direction and velocity of storms are determined by the mean motion of the entire atmosphere in which they exist; and Gen. Greely has recently, in the *American Meteorological Journal*, educed the recorded wind-velocities on Mount Washington as favoring this view.

In order to study this and allied questions, the writer began two years ago a systematic series of observations on the clouds. These observations were made hourly between 7 A.M. and 11 P.M. Facilities were not available for obtaining the actual velocities of the clouds, and it was hence necessary to be content with obtaining the apparent velocities. These were obtained by means of a nephoscope devised by the writer. The nephoscope consists of a horizontal mirror held in a frame carrying an eye-piece movable along vertical and horizontal arches, so that the direction of cloud-movements can be determined in degrees of azimuth. To obtain the relative velocity, a movable support is so arranged, that, when the observer's forehead is rested on it, the retina of the eye is maintained at a constant height of seven inches above the surface of the mirror. When the eye is in this position, the number of quarter-inches which the image of a cloud is seen to move across the mirror in a minute is taken as the relative velocity of the cloud. It is evident that the relative velocity of the cloud thus obtained bears a relation to the actual velocity; and, if the height of the cloud be known, its absolute velocity relative to the earth's surface



can be calculated. From measurements made at Upsala and elsewhere, it seems safe to assume that the average height of the highest form of clouds, which are known as cirrus and cirro-stratus, is 25,000 feet as a minimum estimate; and on this basis it is calculated by the writer, according to known geometrical principles, that, if the relative velocities obtained by him for this form of cloud be multiplied by ten, they will represent approximate absolute velocities. This, however, does not alter, but merely increases, the number of divisions in the scale used for the observed relative velocities; and, even if the attempt to express the results in approximate absolute velocities is erroneous, it does not alter the main conclusions of the following article, which are based on the measured apparent velocities of the cirrus as seen from the earth's surface.

Observations on the relative velocity of the clouds were begun in February, 1887; and during the two years 1887 and 1888 there were obtained 1,821 observations of the relative velocity of the cirrus level of clouds, distributed as follows: —

	1887.		1888.	
	No Days.	No. Observ.	No. Days.	No Observ.
January	0	0	19	58
February	3	12	18	74
March	11	39	18	59
April	21	61	19	89
May	23	131	16	91
June	21	124	23	163
July	25	107	22	117
August	19	102	2	110
September	10	44	13	73
October	17	78	14	54
November	19	89	15	49
December	10	33	18	64
Year	179	820	217	1001

To correct for irregularities in the intervals between observations, the months were divided into six periods of about five days each, and averages for each period determined. From the average of these the monthly average was obtained. During the two years there were only three of these periods of five days within which no observations of the cirrus velocity were obtained.

The following numbers obtained as stated above express approximately in miles per hour the average monthly velocity of the cirrus obtained during two years (February, 1887, to January, 1889): January, 120; February, 106; March, 80; April, 85; May, 67; June, 58; July, 57; August, 64; September, 60; October, 81; November, 81; December, 102; year, 80.

Individual velocities exceeding 200 miles per hour were not uncommon in the winter months; and, even if these very rapidly moving cirri did not exceed 20,000 feet in height, their velocities must have been greater than 150 miles. In the accompanying diagram are plotted for each month during nearly two years the following data: the average monthly storm-velocities in the United States, as obtained from the United States Signal Service *Weather Review;* the average monthly velocity of the cirrus observed at Blue Hill Observatory; the average wind-velocity obtained during a part of this time on Mount Washington; the average wind-velocity obtained at Blue Hill Observatory; and the average westerly component of the Blue Hill wind-velocity. The westerly component was calculated by multiplying the north-west and south-west winds observed at Blue Hill by cos 45°, and adding the results to the wind-movement from the west, then subtracting from this the easterly wind-movements treated in the same manner. The result gave the excess of the westerly component of the atmospheric movement.

The observed direction of the cirrus movement was almost invariably from some westerly point, movements from the east only being observed on about a dozen days during the two years: hence no correction for direction was attempted.

It is seen that all of the curves follow the same general sweep, indicating that the velocity of storms is intimately related to the velocity of movement of the general atmosphere; but the most intimate relation between the two is evidently in the cirrus region. The curves show that almost every increase or decrease in cirrus-velocity was coincident with a corresponding increase or decrease of storm-velocity. The first letters of the months are placed along the curves so that the corresponding parts can be more easily followed. In general, the minor oscillations of the curves representing the lower winds were in opposite direction to those of the cirrus; but it seems worthy of notice that the only part of the storm-curve which differed from the cirrus-curve followed very closely the curve showing the westerly movement of the wind at Blue Hill.

In order to determine whether the relation between the upper air movements and the storm-velocity was a constant, the months which showed average cirrus-velocities between 30 and 50 miles per hour, were combined and averaged, as were also the average storm-velocities for these same months. In the same manner the cirrus-velocities between 50 and 70 miles, and the storm-velocities for the same months were averaged; and so for each 20 miles of increased velocity of the cirrus-level. The following table gives the results: —

Cirrus-velocity	30–50	50–70	70–90	90–110	130–150
Average	44	61	80	94	136
Storm-velocity	20	25	30	32	34
Ratio	2.2	2.4	2.7	2.9	4.0

This shows that the cirrus-velocities increased more rapidly than the storm-velocities. A more detailed study, which is not given here, showed that this held true for winter as well as summer, and also showed that the most frequently observed cirrus-velocity was about 60 miles per hour.

A similarly prepared table showed that the storm-velocity increased more rapidly than the wind-velocity at Blue Hill; while the ratio between the two at the height of Mount Washington seems to be almost a constant (see *American Meteorological Journal* for December, 1888.

The following table, prepared in the same manner as that above, shows how intimately also the variability of the weather is connected with the velocity of the cirrus. The variability of the pressure and temperature at Blue Hill Observatory was found by ascertaining how much the means of consecutive days differed from each other, and averaging the results without regard to sign. The variability of rain was calculated on the basis that rain on every alternate day would make 100 per cent: —

Cirrus-velocity	30–50	50–70	70–90	90–110	130–150
Average (in miles)	44	61	80	94	136
Mean daily change in pressure	0.11 in.	0.12 in.	0.18 in.	0.19 in.	0.23 in.
Mean daily change in temperature	3.8°	4.2°	5.3°	6.4°	7.8°
Rain variability (in per cent)	19	37	39	31	51

There has been a striking contrast between the velocity of the cirrus observed during the winter of 1887–88, and during the winter of 1888–89 up to the present time; and this is no doubt correlated with a striking contrast in the distribution of temperature during the two winters. During the winter of 1887–88 the temperature in the northern part of the United States was decidedly below normal, while in the southern part it was above. This, no doubt, very much increased the normal pressure-gradient from the equator toward the pole in the upper air; and as a consequence the upper-air movement was very rapid, carrying the cyclonic eddies along with exceptional rapidity, and causing rapid and violent fluctuations in the temperature, rainfall, humidity, etc., over the entire United States except the Pacific coast. On the other hand, during

the winter of 1888–89 the temperature has been decidedly above the normal in the northern United States, and normal or below normal in the Southern States. As a consequence the pressure-gradient in the upper air has been less steep than usual, the movements of the upper-air currents and of storms has been comparatively slow, and the winter over the entire country exceptionally free from sudden changes. The correlation of these facts seems to the writer to promise much; for, when the causes governing the distribution of temperature are better understood, it seems evident that the meteorologist will be able to foretell for considerable intervals the special characteristics of the weather to be expected over large areas.

I trust these few facts may serve to further stimulate the interest which is now being aroused in more exact and detailed cloud-observations. H. HELM CLAYTON.

Blue Hill Observatory, Readville, Mass., March 20.

The Robinson Anemometer Factor.

THIS name has been commonly applied to the earliest expression of the law of relation between the velocities of the centres of the cups of the Robinson anemometer and that of the wind which sets them in rotation. Being a simple ratio between the two velocities in question, or $\frac{w}{v}$ as expressed in *Science* (xiii. p. 227), it is not surprising that subsequent experiments should show, that not only is the original factor, namely 3, incorrect, but that such a simple relation can by no means be made to express with any reasonable accuracy the anemometer law. It is surprising, however, considering the numerous experiments made by Dohrandt as well as others, that writers and investigators of the present day should still adhere to the use of the old anemometer "factor," and group together in a general mean a large number of experiments at different velocities.

The writer of the communication referred to above, in a discussion as to just what constitutes the true anemometer factor, has presented the matter in a form that shows at once how futile it is to use the old factor. Following Dohrandt and others, he assumes that the velocity of the wind, w, and the velocity of the cup-centres, v, bear the following relation to each other: —

$$w = a + bv \quad (1),$$

in which a and b are constants. The anemometer factor then becomes

$$x = \frac{w\,b}{w - a}.$$

We see from this equation, that, when $w = a$, x becomes infinite, which corresponds to the condition when the wind is just too feeble to start the cups. As w increases, x approximates more and more to the value of b; but, even between the small ranges of velocities that occur in ordinary practice, x is entirely too variable to consider constant, as shown by the values given in the above-mentioned paper, and is too troublesome to use in calculation, especially since the very equation from which it is computed is in much simpler form, and, moreover, gives at once the velocity of the wind from the cup-velocity, which is the quantity observed when the anemometer is in use. The facts of the case, however, are not satisfied with even this degree of complication, and the anemometer factor becomes quite out of the question. Dohrandt's results up to 30 miles per hour are only approximately represented by an equation like (1); and as in his experiments, owing to the comparative shortness of the whirling arm, the friction of the anemometer at high velocities, from centrifugal action, was very great, it may be shown that the approximation is even closer than it actually should be. In fact, recent anemometer experiments upon a whirling arm 35 feet long are in most cases represented accurately by an equation of three terms: thus,

$$w = 0\,225 + 3.14v - 0.0362\,v^2 \quad (2),$$

the numerical values being those computed for an anemometer of the Signal Service pattern, the cups of which are 4 inches in diameter on arms 6.7 inches long.

In view of this discussion, and taking into consideration that the experiments just mentioned were made in a closed court under the most favorable circumstances, it would appear that the different conclusions reached by the wind force committee of the Royal Meteorological Society in their open-air experiments are largely misleading and in error, due probably to the serious influence of the outside wind-movement.

It seems that one effect of this wind-movement, outside of the motion of the arms of the whirling-machine, is not clearly understood, or at least receives little attention, and is nevertheless of the greatest importance.

If a uniform wind blows across the path of the anemometer when being carried upon the whirling-machine, every one sees, that, during one half of its motion, the anemometer is going more or less with the wind, and against it during the remaining half. That these effects do not fully neutralize each other, is clearly shown in a mathematical analysis by which it is not difficult to find the correction that should be applied; but this is only small in most cases, and is not very serious. A far greater error arises from the effect this extra wind has in causing a very large and rapid variation in the actual wind-movement experienced by the anemometer, which, if its axis is being revolved on the whirler at the rate of 15 miles an hour, and an extra wind of 4 miles per hour is blowing, is at one point of its path moving through the air at the rate of 19 miles an hour, and at the opposite point at the rate of only 11 miles per hour; the change, moreover, from the maximum to the minimum being accomplished with great rapidity. The mean velocity of the cups in this case may be shown to be such as corresponds to a wind-velocity of nearly 19 miles per hour, the reason being that the inertia of the cups keeps them spinning after experiencing the maximum velocity; so that during the minimum velocity they do not slow up as they should, the only tendency to do this being the air resistance to the backs of the cups; and, as this is considerably less than that felt by the front or concave sides of the cups when the wind tends to increase their velocity, it must follow that the mean velocity of the cups in a variable current is considerably higher than such as would otherwise occur. A more extended statement of this inertia effect, and numerous experiments by which the theory is confirmed, have been already submitted for publication in the *American Meteorological Journal.*

The large and erratic variations in the results obtained by the wind force committee with anemometers of the Robinson type are to be attributed to this cause; and the noticeably more uniform results obtained with the helicoid anemometer were due to the fact that this instrument, being driven by the direct pressure of the wind, and not by the difference of several pressures as is the case with the Robinson anemometer, is not subject to the inertia effect just described. The explanation of this point, given in *Science* of March 22, p. 227, to the effect that the helicoid anemometer was tested with a vane attached to keep it in the wind, is hardly sufficient to account for its seeming better performance.

It follows from the above, that, if two sets of anemometer cups are fitted up exactly alike except in weight, one having paper cups, for instance, the latter will in the open air, exposed to a variable wind, give seemingly less wind than the former, both being reduced by the same formula. Formula (2) given above is also only to be used for perfectly uniform currents.

Some mention was made in a "Note on the Robinson Anemometer Constant," in *Science* of March 15, of the relative merits of the recently invented helicoid anemometer and those of the Robinson type. Judging by the description of the former, its mechanical construction cannot possibly be so simple as that of the latter; and as to what would happen to it and its delicate self-adjusting vanes when exposed to the sleet and frost of a winter season, is by no means difficult to tell. The inventor himself considers the instrument defective or unsatisfactory, owing to the ease with which the readings are altered by bending the vanes.

Robinson anemometers, to give the most satisfactory results in the open air, and variable winds, should have very light cups.

It may be added, in conclusion, that all anemometers acting by direct wind-pressure are subject in much greater degree to variations in their law connected with temperature and pressure changes than are those depending only on difference of pressures.

C. F. MARVIN

Washington, D.C., March 25.